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THE INSTITUTE OF ACTUARIES.

*Account of a Correspondence between Mr. George Barrett and
Mr. Francis Baily. By PROFESSOR DE MORGAN.*

IN looking over some papers of the late Francis Baily, at the request of his sister, for the purpose of advising on their disposal, I found in one packet a number of letters addressed to him by the late George Barrett, the inventor (in this country) of the method for calculating life contingencies which now goes by his name. I also found the manuscripts of Baily's three works—on leases, on interest, and on life assurances: these are now in their proper place, the library of the Institute of Actuaries.

From Baily's methodical habit of arranging his papers, I feel confident that the letters alluded to are all which he received from Barrett: unless, indeed, some passed which he preferred not to preserve. The interest with which actuaries look upon the method *published** in these letters makes me think it desirable to give some account of them in a work which all actuaries consult.

I have also been able to obtain† some information as to Barrett's life.

* I here use the word in its old and strict sense. Persons who have dipped into the history of discovery have often seen the *rule of first publication* cited as *Waring's rule*; and sometimes publication is limited, upon Waring's authority, to circulation by the press. But Waring's words are—" *Is mihi semper dicendus est inventor, qui primus evulgavit, vel saltem cum amicis communicavit.*" Of course I do not acknowledge any authority here: I only meet those who do.

† For this I applied to Colonel Wyndham, the possessor of Petworth House, who obligingly introduced me to the Rev. John Peachy, the husband of Barrett's niece. To Mr. and Mrs. Peachy I am indebted for nearly all the biographical facts, construction and comment being of course my own.

George Barrett was the second son of a respectable farmer or yeoman, who lived at Wheeler Street, a small hamlet in the parish of Witley, Surrey. He was born about 1752. At an early age he was possessed by a great love of calculation, and spent all the money he could get on books of arithmetic, algebra, &c. It is said that, when a boy, he was one day met by the Vicar of his parish, who was in difficulty about some matter of calculation, but was extricated by his young parishioner, who levelled the sand near him with his hand, and worked out the question on it with perfect accuracy. Finding that he could not commit his thoughts to paper with facility, he persuaded his father to send him for a short time to a school in the neighbouring town of Godalming, to improve his handwriting. When he had been there a very short time, his master said to him one day, "Would you not like to learn a little ciphering, George? I would not give up all my time merely to learn writing." George did not seem at all desirous of profiting by the offered assistance, but consented, on the offer being repeated. Sums were given him from simple addition upwards, until the ability of the master was exhausted. "And now, Sir," said the boy, "will you give me leave to set you a sum?"—"Oh, yes, George, by all means." This was done, and the master acknowledged that he could not work it; adding, "If I were you, George, I would take off that round frock, and apply my mind to a more suitable occupation."

It is also said of him, that at 17 years of age, while sitting under a tree, he "discovered the distance of the earth from the sun"—meaning, no doubt, that he thought of some *method* for doing this. A modification of the steam engine (in which he afterwards found he was not original) is attributed to him; and the telegraph was an idea of his, derived without knowledge of any other inventor, if indeed any other inventor had preceded him. On this subject he communicated with the Government, and had reason given to expect employment, if any telegraph should be brought into use. Here he was disappointed; and his own suspicion was, that his revelations to another person had enabled that person to pass for an inventor. This is a matter which of course I cannot enter upon; I only mention it because his communication with those in power explains how it was that he afterwards went to Dublin for a short time on some Government employment, of the nature of which no details are preserved. Shortly after this, the late Earl of Egremont offered Barrett a situation in his household at Petworth; the date of this appointment is not exactly known,

but he is supposed to have been in the Earl's service in 1801. From the correspondence with Baily, he seems to have been a steward, bailiff, or managing agent of some kind—at least in 1811. Of this correspondence, and its object, I shall presently have to speak.

After his name had been made known to actuaries by Baily's *Appendix*, in September, 1813, he was appointed actuary to the Hope Assurance Office; but was informed—I think about December, 1815—that his services were no longer required. A letter to the directors, of the last date (which, however, does not explain anything sufficiently), imputes their “late determination,” which I suppose to mean the dismissal, to some dissatisfaction at the rate with which certain calculations were proceeding.

He retired to his sister's house at Godalming, worn out in mind and body, a martyr to the gout, and feeling deeply the failure of his attempts to publish his Life Annuity Tables. He died April 21, 1821, after months of extreme suffering, and was buried in Witley churchyard. He was during a great part of his life a frequent invalid, from gout and other disorders.

Barrett was by nature a *calculator*, and subject to the absence of mind which often accompanies *numerical* thought. He scarcely took a meal without marking figures with his finger on the table. He was known occasionally to forget to sign his name to his own letter; and this circumstance, in the neighbourhood, was considered as a sufficient identification of the writer. Though a man of strong feelings of affection, he was never married. His conduct towards his relatives was marked by the truly paternal kindness with which he treated the orphan children of one of his brothers. Nor was this all. His father had made his will before the two youngest children were born, and had not time, in his last illness, to make any codicil in their favour. But his son George promised that he himself would give to those children the same sums which had been left to the daughters; and this promise was fully kept, though it obliged him to sell the small property to which he had succeeded.

The mass of tables of which I shall presently speak were bought, a few years after Barrett's death, by Mr. Babbage, in whose possession they now are. They were constructed by Barrett himself up to the transfer of the products, found by logarithms, into numbers: in the additions and transcriptions he was assisted by his sister and niece, and (for a short time) by another niece and two nephews; and by no other persons. It was the earnest

desire of his family that he should write a treatise on his own method, but this his infirmities prevented. Some time after his death, most of his papers were accidentally destroyed by fire.

He never learned either French or German; but as he at one time kept a school, his friends suppose he had some knowledge of Greek and Latin. This seems to me not at all probable, his early education and subsequent pursuits being remembered; nor can any trace of such knowledge be detected in his letters. His books, so far as I have had their titles named, were English. The copies of a pamphlet which he once printed on the state of the police, were lost with his papers. He wrote occasionally, it is thought, in newspapers, under the signature G. B.: copies of letters to the editor of the *St. James's Chronicle* (one of August, 1783) have been found among his papers. His peculiar talent was never of any permanent use to him in a pecuniary point of view; and it seemed as if, to use his own words, "Thou shalt not prosper" was written on all his undertakings.

The mass of tables now in the possession of Mr. Babbage (including one of *Three Joint Lives*) was principally constructed between 1801 and 1813. During the greater part of this period a certain number of papers were sent weekly by Barrett from Petworth to Godalming, for his niece (who gave four hours a day, on the average) to work on, and for his sister to examine, previously to the whole receiving the final revision from himself.

Barrett's first communication to Baily is dated March 10, 1811. Referring to the *Treatise on Life Annuities*, then recently published, he expresses a desire for more numerical solution, and thinks it possible that Baily was deterred by want of sufficient extent of tables. He then states that for some years he has had ready for press an extensive set of tables, but doubts whether they can be published except by subscription. He gives a list of these tables, as follows:—

I. Sweden expectation of Two Joint Lives, for all differences of age.

II. For easily finding the expectation of One, Two, Three, or Four Joint Lives, from De Moivre's hypothesis, "the utmost extent of life may be taken as high as you please, not exceeding 96."

III. Four per cent. Male Single Life Annuity Tables.

IV. Ditto Female.

V. Sweden Single Life Annuity, at 2, $2\frac{1}{2}$, 3, $3\frac{1}{2}$, up to 10 per cent. "The last three tables will also show the value of an annuity, during any given life, of £1 payable at the end of the first year, £2 at the end of the second year . . ."

VI. Four per cent. Joint Life (one male and one female) Annuity Table, for all ages; also showing the increasing annuity.

VII. Sweden Joint Life Annuity Table, for all ages, at 3, 4, 5, and 6 per cent. Mr. Dutens (the well known editor of Leibnitz, and writer on the discoveries of the ancients) sent a part of this table to Baron Maseres, which accidentally never came to hand, so that a duplicate* was sent. "However, this untoward circumstance will be a precaution not to trust in future any of my papers in the hands of gentlemen, let them be ever so respectable."

VIII. One, Two, Three, and Four Joint Lives, from De Moivre's hypothesis, at 2, $2\frac{1}{2}$, 3, up to 10 per cent. "The Four Joint Lives might have been excluded as useless, but are retained to exemplify more fully the method of calculation. By this table I can find the value of an annuity on the joint lives in a few minutes, either for terms or the whole duration of the lives."

IX. Four per cent. [Swedish] Table on Three Joint Lives. "This table will be comprised in 672 pages quarto . . . Mr. Morgan has expressed an opinion that a work to that extent would never be accomplished from a table of real observation." "N. B. All the above tables show the value of the annuities, whether they be temporary, deferred, or for the whole duration of the lives, and at any difference of age; and therefore you well know that the labour in constructing them must have been immense. The above is all I mean to say to you on the subject at present; if you should be of opinion that the tables will be of public utility and the calculations to be depended on, you will be so good as to communicate your sentiments on the subject . . . I take this opportunity of informing you that your rule for approximating to the value of ρ (see note A, p. 122, line 3) in your *Annuities*, will be greatly improved by substituting for $\frac{s}{a}$ (at the end of the denominator) the amount of £1 *per annum* for n years, at the assumed rate. Put therefore 79·05818622 for 80 in the example below, which is my method of approximation."

When it is considered that Barrett does not hint at any novelty of method, and that he implies that his table *shows* all annuities,

* Baron Maseres laid the scientific world under many obligations by his reprints, and by his assistance to authors who could not afford to print for themselves. It is a great pity that he did not patronise Barrett. The *Scriptores Logarithmici* has been very useful; but matter amounting to two volumes out of the six might easily be spared, and those two volumes form a bulk which would have contained Barrett's tables, even up to Three Joint Lives, for one rate of interest at least. I confess that I sometimes look into the mouths of the Baron's gift horses.

temporary and deferred, on every combination of *three* joint lives, the readers of this article will certainly give Baily credit for great politeness or great sagacity, when they are told that he replied by expressing his willingness to promote the subscription, adding some remarks on the desirableness of abandoning De Moivre's hypothesis in favour of real observations. I suspect that at this time the prominent idea in Barrett's mind was, not the method itself, its goodness, and its novelty, but the vast mass of tables which it had enabled him to construct: his expressions show that he thought more of his work than his tool. It is one more instance added to the hundreds which I have noticed, of the difficulty which the isolated and self-taught man has in taking his subject from more than one point of view.

In his second letter (April 28, 1811), after acknowledgments and some remark on De Moivre's method, he proceeds thus:—"I must now beg leave to inform you that my tables are constructed in a way different from any I have before seen; had that not been the case, I verily believe the public would have always been in want of a table of three joint lives for all ages, and therefore this must be my apology for adopting a method different from all others; and I hope it will prove to you satisfactory. If any will object to it, let them try their skill, or rather their patience, in producing tables equally comprehensive, that may be found more consonant to their wishes: I shall certainly have no objection. I am fearful I was a little unguarded in my expressions when I said '*tables showing*,' &c., when it would have been more appropriate to have said, '*tables for finding, or determining*, &c.' For the value of an annuity *deferred*, or for the *whole of the same life*, is expressed in my tables by vulgar fractions having each the same denominator, and therefore a division is requisite to reduce them to a decimal form: they therefore cannot strictly be said to *show* those values, by inspection, *decimally*; but they show the values of the annuities, whether for the whole life or when deferred, by a *vulgar fraction*, and consequently, to find the value of a temporary annuity, one subtraction and a division will be necessary; and as to *increasing* annuities, the values thereof, whether temporary, deferred, or for the whole duration of the lives, will be determined in like manner."

Baily has marked the difference of the footing on which his correspondent now came before him, by preserving a copy of his answer (May 4, 1811). The following is an extract:—"You observe that your tables are constructed in a very different manner from any others; and I confess I cannot make out, from anything

you have stated, the method which you have adopted on this occasion. I make no doubt, from your knowledge of the subject, that they are properly constructed, and are very probably rendered as convenient as the present state of the science will admit. Nevertheless, as their form certainly varies from any yet in use, and as their principle of construction may admit of some additional examination, I do not think I am properly qualified at present to give a decided opinion on their merits or demerits. If, however, you are willing to send me a specimen of each of your tables, together with the principles of their construction, I shall very readily examine them, and will give you my free opinion on their advantages and utility; and should that opinion (which I anticipate) be confirmed, I shall be happy in promoting the plan which you have adopted for publishing them."

Barrett replies (May 7) by requesting leave to wait on Baily with his papers. This interview seems to have taken place, and to have ended in a promise to forward a written account; for (June 11) Barrett, with apology for delay arising from business, Lord Egremont being at Petworth when he *returned*, forwards a paper to explain his principles, of which the following is an abstract:—The method of finding the expectation of one life, and of two and three joint lives, is deduced at length. Tables are suggested having the ages, the numbers living (or their products for two or more lives), and the sums of the living, at and above each age. The method of finding the value of an annuity for any term is fully deduced (the self-taught student, who has not been in communication with other students, has but little idea of what he may omit in addressing those as well informed as himself). This value is thrown into the form $br^{n-1} + cr^{n-2} + \dots$ divided by ar^n , r being the amount of £1 at the end of one year. The column of ar^n is suggested, the column of the sums, and the column of the sums of sums for increasing annuities. The deductions are, the value of the whole life, the temporary and the deferred annuity, and the annual premium for a deferred annuity, payable till it commences. The rule of formation for two or more lives is described. This paper, dated June 11, 1811, is Barrett's first extant publication of his method, until (should that ever happen) the papers forwarded to Baron Maseres make their appearance. A detached specimen of the two first columns, for joint lives, is, I presume, a part of it; but it is not dated.

Baily's reply (June 17, 1811) is the letter which was published with the prospectus announcing the publication by subscription.

It gives some description, and strong praise. A postscript, which was not printed, gives some advice on publishing details, and renews the offer of assistance, especially in the arrangement of the explanations.

This prospectus was printed, but not circulated, by September 2; as appears by a letter of that date. The prospectus had stated that the mass of the tables, after the proposed selections had been printed, were to be deposited in some public institution, "for the benefit of those who may be disposed to pursue the inquiry further." This Barrett's friends thought would give a title to anyone to publish these tables, from which, at that time, he thought a profit might be derived. He accordingly proposed to cancel the words. On September 5, he acknowledges a letter correcting his erroneous impression about the copyright, and, still under the idea of a large subscription, proposed sending a prospectus to every member of both Houses of Parliament.

Next follows a circular addressed (Sept. 24) by Baily to fifteen Assurance Offices, claiming their support on the merits of the case, not merely by the order of a single copy, which it is assumed they must buy when it appears, but by a more liberal encouragement. This letter states that Barrett had been engaged for twenty-five years in his undertaking. Five answers are preserved. The directors of the Albion, "fully disposed to encourage any publications containing new and correct exemplifications of the subjects to which their business is directed," subscribed for six copies; but hinted that the Table of Three Joint Lives, not being Northampton 3 per cent., would be of no use to them. The Provident subscribed for four copies, with a similar hint; to which it was added, that Northampton 3 per cent. approximations from Barrett's Swedish Four per Cent. Table would add but little to the accuracy of existing methods. Mr. Milne, from the Sun (which Baily has forgotten in the account given in his *Appendix*), replied that he had referred the prospectus to the managers. The Atlas declined altogether, and stated that the Office was already in possession of every possible combination of two lives at 3 per cent. Mr. Baily has stated that the Westminster absolutely refused to subscribe for a single copy. This is not correct, and probably arises from his eye catching the words "consequently cannot subscribe," near the bottom of a page, when he was writing his enumeration. The answer of this Office raises a distinction which is worth attention. "There have been proposals on the subject of the same nature many times before the public, which have either been incomplete or erroneous

when carried into execution, and sometimes have gone no further than the subscription. The directors are not averse to the publishing tables, but the contrary; but they cannot put the name of the Office to what they are unacquainted with, and consequently cannot subscribe. Mr. Barrett may however be certain that, should he go on with the work, the Office will purchase at least one of the copies." Perhaps the Office might, without much departure from a very sound and difficult principle, have accepted the guarantee of the author of the *Treatise on Annuities*, which was then in its early fame. But this was quite a matter of discretion; while, as to their main reason, it is one of which every person and institution must have felt the force. *Subscribe* and *underwrite* are the same words, and are very often held to have the same meaning.

By October 21, Barrett began to see that the subscription would not succeed. His application to the members of the House of Lords had produced but four peers—Devonshire, Headfort, Cholmondeley, Romney; he had also one commoner, the well known, eccentric, but most worthy and amiable, Jack (it is impossible to say John) Fuller.

There is nothing further till January 4, 1812, when Barrett writes, in his postscript, that he has given up his intention of publishing his work. The letter itself mentions some theorem in prime numbers which had been transmitted to Mr. Barlow, and gives the following formulæ, which are worth preserving as curiosities:—

If n be the oldest age in the tables, and there be three joint lives (the annexed values being for $n=96$, as in the Northampton Table), the following are the number of cases under the heads mentioned:—

$$\text{Whole life annuities, } \frac{n^3 + 3n^2 + 2n}{6} = 152096;$$

$$\text{Immediate temporary annuities, } \frac{n^4 + 2n^3 - n^2 - 2n}{24} = 3612280;$$

$$\text{Deferred remainder of life annuities, } \text{the same;}$$

$$\text{Deferred temporary annuities, } \frac{n^5 - 5n^3 + 4n}{120} = 67910864;$$

$$\text{All sorts together, } \frac{n^5 + 10n^4 + 35n^3 + 50n^2 + 24n}{120} = 75287520.$$

Mr. Barrett adds—"You may make what use you like of the above rules. I suppose the *Appendix* is by this time gone to the press, and therefore you will probably receive them too late to

insert at the end thereof." Baily's actual *Appendix*, as my readers know, is the paper rejected by the Royal Society. It thus appears that an appendix was the original intention, that the offer of a memoir to the Royal Society was a change of intention, and that the rejection of the offer was the cause of the return to the original plan.

This paper to be offered to the Royal Society is first mentioned as in agitation in the next letters, March 27, and April 9 and 22, 1812, in the first of which the intention of sending prospectuses to the members of the House of Commons is alluded to, and also that of publishing only the first volume, if subscribers enough should not be obtained for both. It is also mentioned that proposals had been sent to all the Universities. There was in 1814 a committee of agitation against the claim to eleven copies of every work published. This committee printed a letter from Barrett to the late Davies Gilbert (then Giddy), who had presented a petition from them to the House of Commons. This letter states that the answer of the University of Oxford was, *that the University had a right to a copy gratis; and as it was a book of reference, this one would serve all the Colleges*. The request for a subscription was not on common grounds; it was made on the plea of merit, which could not, on the most sanguine calculation, procure the means of publication except by subscription. The University, or its officers, would have done more wisely if they had kept back their reason; unless, indeed, they meant to damage their own title.

May, 7, 1812, Barrett expresses great pleasure at his employer, Lord Egremont, having become a subscriber; the want of this name had, naturally enough, been a sore subject in several of the previous letters.

May 17, Barrett expresses his pleasure with the draft of the communication to the Royal Society; and refers to the calculating rods which are described in Baily's *Appendix*. A postscript says, "By the enclosed letter of Dr. Price you will see that I had an idea of solving all the questions relating to contingent assurance previous to the publication of Mr. Morgan's papers on the subject." He reminds Baily more than once to take care of this letter.

August 22, 1812, Barrett refers to the rejection of the paper by the Royal Society, and desires to know who formed the committee of reference. This letter contains inquiries about Zerah Colburn, in whom Baily was then much interested. The same subject is resumed in September 4, 1812.

On December 11, 1812, Barrett announces that he will not

then make any further attempt to publish, but will complete the work, and leave it to his three nieces.

December 20, 1812, Barrett communicates the method of proving the logarithmic process which is published in the *Appendix* (p. 42).

February 15, 1813, he makes some remarks on the proof sheet of the *Appendix*.

March 21, 1813, he gives a graphic account of a family consultation on a proposal of Baily that some small part of the tables should be separately published; which proposal was totally and unanimously rejected.

September 19, 1813, Barrett announces the completion of his arrangements with some [the Hope] Life Office, and that he is to enter upon his duties on the 29th. This letter is the last of the series: there is one more (February 19, 1819), on a method of finding the longitude, and also a letter of Mr. Barlow's, containing reasons why it could not be efficient.

The rejection of Baily's paper on Barrett's method by the Royal Society is one of those unfortunate instances which create a fear lest there should be other communications, as valuable, which have also been rejected, but have never found such a champion as Baily. It is usual to attribute this rejection to the late William Morgan, who was at the time a member of the Council, and must doubtless have been on the committee of reference, unless his own sense of his peculiar position with respect to his assailant induced him to decline the office. But it must not be forgotten that the celebrated Thomas Young, an acute writer on annuities, was also on the Council, and as probably on the committee. Baily, who at the time I write of was in the habit of expressing himself with great force and plainness on all matters of scientific difference, was afterwards, as it happened, in open opposition to Young on the question of the *Nautical Almanac*, as he had been to Morgan on that of the correctness of annuity formulæ. No publication relative to the *Nautical Almanac* had been made by him in 1812. Morgan and Price, as is well known, had at one time possessed a mastery over the subject of life contingencies which was almost (may I not say, quite?) peculiar to themselves. Morgan himself, in spite of the occasional errors so sharply attacked by Baily, had greatly contributed to the advance of the science by his papers, and to the establishment of public confidence in it by his management of the Equitable Society. I think it may be said to be pretty well known that he had acquired a kind of feeling, that to meddle with the

subject of annuities and assurances was to poach on his manor. This weakness may have biassed him in his judgment of a new and strange method; but he must not bear the blame alone: it is the business of a society to counteract the known bias of each individual member. Strong sympathies and antipathies are the frequent concomitants of energy; it is the duty of colleagues to prevent them from unduly influencing joint conclusions. In the present case, it was notorious that the author of the paper under discussion had given great offence to the member of the Council who, under ordinary circumstances, would have been the best judge of its merit. If the remembrance of that offence contributed to the rejection of the paper, the parties who permitted the result were more to blame than the individual whose natural anger had originated it.

But I must be allowed to state my own impression, that, though Morgan had not been on the Council, the rejection would still have taken place. It appears to me that the Royal Society, during the present century, has shown great want of power to appreciate improvements in calculation of results; and I am afraid I must add, that the University to which I owe my own education has been one cause of this exhibition. I think that for fifty years there was a growing tendency at Cambridge to neglect, in teaching, all that follows the resulting formula or the final equation; though I suspect that this tendency has passed its culminating point.

The first Number of this *Journal*, by a judicious use of a fortunate coincidence, opened with an historical paper, in which, for the first time in England, it was made known that the method separately invented and practised by Barrett had been published by Tetens in 1785. I may refer to the paper by Mr. Hendriks for a full account of the German publication.

It is as certain as anything can be that Barrett was a perfectly independent inventor. He knew neither French nor German; his own method was described by himself in 1811, when there was no question of a competitor, as having been worked at for 25 years, which brings the invention back to about the time when Tetens published. In our own day, a discovery made in Germany, and attracting considerable notice in that country, may have lived for years before it comes to the knowledge of an investigator living in the scientific society of London, unless he be a special cultivator of the German language, with opportunities of seeing literary notices in that language. The chance of a work like that of Tetens finding its way into the hands of a Surrey yeoman farmer or country schoolmaster, in or about 1786, is very small indeed. Further,

the work of Tetens acquired no notoriety in England. Milne, who knew the continental authors better than any of his contemporary writers, does not allude to it in the summary given in the article 'Annuities' in the *Encyclopædia Britannica*; nor is the book itself, to a cursory examiner, suggestive of anything new. I have had it in my possession for twenty years; but, not reading German with anything the least approaching to fluency, and seeing by the commencement and by the tables *at the end** of the first volume that the work seemed mainly intended to combine the recent English writings with those already known in Germany, I never attempted any close examination of it.

Tetens, as Mr. Hendriks has remarked, described the use of the columns C and M; which Barrett† did not do. It may reasonably be supposed that he saw the application of his own method to any question which is solved by the form $br^{n-1} + cr^{n-2} + \dots$ divided by ar^n ; but he makes no incidental remarks: even when he forms his columns for the expectation of life, in explaining his method to Baily, he does not drop a word on the facility with which the temporary or deferred expectation may be found. The points of view of the two men differed greatly. Barrett was occupied with the production of vast results, and valued his method because it produced those results; Tetens exhibits an easier method of doing what others had already done, and does not seem greatly struck with the power of doing more which that method would give. Hence, probably, the reason why Barrett did not care for such columns as C and M and R. He could not hope to repeat them on a table of three joint lives: so that he was contented with the facilities which his own D and N columns gave to problems of reversions, as well as to problems of annuities. If a table of three lives were now undertaken, we may be sure that the D and N columns would be all that would be contemplated; and even in the case of two lives Mr. David Jones has (most judiciously) not attempted more.

Again, part of the merit of an invention consists in its mode of introduction. Barrett introduced his method by using the proper means: Tetens so completely failed in introduction, by not using the proper means, that in all probability he never would have been heard of in the matter, if it had not been for Barrett's success. If Barrett had gone to Baily with only a new method, it is very

* If Tetens had put the table of his page 89 at the end of the volume with the rest, his claims would probably have had earlier notice given of them.

† The M column was invented, in this country, by Mr. Griffith Davies.

possible that he might have failed in gaining attention. Ninety-nine men out of a hundred cannot see the advantage of a process which is more simple than their own; merely because, before they have mastered it by practice, it is *not* so simple *to them* as the one in which they have acquired facility. Young students who have learned the ordinary rule for extracting the square root, when first shown how to contract the work and get nearly double the number of root figures, will not unfrequently prefer to work the required number of places by the full method, rather than learn a new process. But when Barrett affirmed the *existence* of a table of three joint lives, which would give even temporary and deferred annuities by at most one subtraction and division, he forced a hearing, and proved that he was a person whose claims it would not be even safe in point of character to neglect; and therefore Bailly kept a copy of his answer to the letter in which this statement was made.

We have here one more instance in which, when the proper time has arrived, discovery suggests itself to more than one person. No one, unless he have attended much to the history of science, knows how very frequently this has happened: it is almost a rule. From the time when Adrianus Romanus and Vieta—one in Belgium, the other in France—were in independent and simultaneous possession of a new development of trigonometry, to that in which Adams and Leverrier were at work upon their memorable problem, and to that in which Lassell in England and Bond in America discovered the eighth satellite of Saturn almost at the same moment, there is a long chain of accordances, the history of which can hardly be well written except in parallel columns.

In the present case, I have no doubt that to Barrett is due an acknowledgment of a much higher order than to Tetens. The first was a self-educated country farmer; the second was a highly cultivated professor of mathematics. The first invented his means of publication; the second used those presented by his position. The first sacrificed a quarter of a century to his determination that the public should not only have the discovery, but the benefit of it; the second gave them the discovery, that those who pleased might benefit the public by it. The first succeeded in making his method of daily use, and of such notoriety that in process of time the discovery of the second was itself discovered; and the discoverer of the discovery deserves no small credit for his unusual research. Nor do I neglect the obvious consideration, that Barrett was in England, where the subject was very much studied, while Tetens

was in Germany, where till lately it was hardly known in its applications. This difference of locality is connected with other points, which are of more importance in an opposite direction. Barrett was in a country in which even obvious improvements upon highly valued arts meet with cold looks and niggard encouragement, when they are not introduced under very influential patronage; he was neglected both by the Royal Society, the Assurance Offices, and the public. Tetens was in a country in which speculation is valued, even in subjects the applications of which are neglected; he occupied honourable and well paid posts in education and in revenue, and died a privy councillor. For all these reasons, I am well satisfied that the important improvement on which I have been writing should, as I am satisfied it will, continue to be called *Barrett's method*.

On the Rates of Mortality prevailing amongst the Male and Female Lives assured in the Eagle Insurance Company during the 44 years ending 31 December, 1851. By CHARLES JELlicoe, Actuary to that Company.

[Read before the Institute of Actuaries, 2nd January, 1854, and ordered by the Council to be printed.]

IN discussing the subject of assurance valuations and the most approved methods of making them, I have often had occasion to draw attention to the great importance of ascertaining with the utmost possible accuracy the rates of mortality and interest which have been found to prevail in any case under consideration, so as to determine what rates of premium are really required to provide, independently of other exigencies, for the sums assured, and how far the particular rates charged are sufficient or more than sufficient for that purpose. I have insisted the more strenuously on the necessity of this proceeding, from the conviction that in almost every Association some peculiarity in the prevailing mortality will be found to exist, distinguishing it from that of its neighbour, and arising from the different character of such influences as the condition of life of the persons whose lives are assured, the mode of selection, the greater or less predominance of the male sex and of assurances made by persons on their own lives over such as are effected on the lives of others, the constitution of the Company, and so on. Almost every Association, differing from the rest in